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Interactive comment on "Towards High Resolution Climate Reconstruction Using an Off-line Data Assimilation and COSMO-CLM 5.00 Model" *by* Bijan Fallah et al.

Anonymous Referee #3

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The paper describes an approach for high-resolution climate reconstruction using an off-line assimilation of proxies into a set of regional climate model simulations. The set-up is tested with the COSMO-CLM model and a number of sensitivity studies are carried out. While the study is interesting, it somehow stops at a point that is still too distant from applications, and it is not clear what sort of applications the authors have in mind. I think the authors should better demonstrate how high-resolution climate reconstruction actually will be obtained and how they will be applied. Furthermore, I found the methodology not very well explained. However, because this is the first paper I am aware of that applies paleo DA to regional climate, I think we can learn a lot and therefore, in my view, the paper is potentially worth being published after major

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revisions.

Major comments:

1) The design of the study is still far from a real world sparse proxy network. Just one example: They assimilate 500 observations, they even go up to 2700 stations and consider 100 "a small number of stations". I would be far more curious to see how the approach works with fewer observations, and what the author's view is concerning other variables (precipitation). Will this eventually work for tree rings? The results section is only 2 pages (part of which, i.e., the localisation, should actually be in the methods section).

2) The paper places itself in the sequence of recent work on paleo DA - it does not mention existing 0.5°-resolved statistical reconstructions. The motivation of many of the global paleo DA studies is to obtain a physically consistent global climate for time periods with spatially very heterogeneous coverage. There are good reasons for suspecting the same on the regional level, particularly for Europe (given the orography and land-sea contrast), but I think this needs to be better justified.

3) The methodology could be explained better. I already stumbled over p.4/l.5, which I first read as impliyng that X_TRUE and X_A is the same (is an "and" missing?). The terms X_NATURE and "free ensemble run" appear before they are introduced. There are some other instances (listed below).

Minor

P. 1, l.12: How can the selection of proxies reduce the background error?

p. 2, l.20: states

p. 3, l.13: Since the sentence cites DA approaches that were actually "applied", it might be good to cite Franke et al. (Scientific Data, 2017).

p. 3, l.20: The sentence is somehow odd: "optimum" in the first part implies a choice,

"truly" implies an esimation.

p. 5, eq. 8: X_NATURE is not introduced yet

p. 5, l. 5: X_Analysis was called X_A before

p. 5, l. 9: X_Analsis -> X_Analysis

p. 5, eq. 11 and 12 are both said to represent "the error covariance of the analysis"

p. 6, l. 9: Is the added noise spatially uncorrelated?

p. 7, l. 14: When describing the shift, the state vector should be defined (because it can no longer include the entire model domain - is it the "evaluation domain", which on my first reading I interpreted as the domain in which evaluations are done).

p. 7, l. 19: The analysis skill should be in the title, and mentioned in the text upfront. Some measure of dispersiveness might be interesting.

p. 7, l. 20: What is a "free ensemble run"? This term is not introduced.

P. 9, l. 13: Please explain the "universal behaviour of fluctuations of terrestrial near-surface temperature"

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